SCIENCE

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THE ALCOHOL PROGRAM OF THE NUTRITION LABORATORY WITH SPECIAL REFERENCE TO PSYCHOLOGICAL EFFECTS OF MODERATE DOSES OF ALCOHOL ON MAN[1]

ALCOHOL in not too large doses, taken by the mouth, is undoubtedly burned in the body and in this burning gives off heat which replaces equivalent energy ordinarily derived from food or body substance. This has been absolutely demonstrated by Professor Atwater and his associates with the respiration calorimeter at Wesleyan University, Middletown, Conn. This scientific proof of the important rôle that moderate doses of alcohol may play in the human energy economy finds verification in the masterly, statistical studies of Armand Gautier in Paris, who has shown that there are certainly several million people who regularly receive in their daily diet somewhat more energy in the form of alcohol than they do in the form of protein. What has been demonstrated of the French is probably true of many others. Thus we see that a physiological study of alcohol is, on abstract, scientific grounds, essential to a complete understanding of the materials regularly ingested which serve as the sources of energy to the body.

Although protein, fat and carbohydrates have long been studied in a systematic manner, alcohol in recent years has, in spite of the agitation regarding its moral, economical and sociological importance, received but scant, irregular attention in a relatively few scientific laboratories. With regard to its physiological action there

[1] An address given before the New York Academy of Medicine on April 6, 1916.

exists much speculation and a minimum of verified facts. This condition should be reversed, and a great deal of hypothesis which, for the most part, lies outside the realm of probability, should be replaced with a careful series of attested facts obtained under conditions insuring the best available technique, with a sufficient number of subjects and a multiplicity of observations accompanied by controls and normals. Such a procedure is outlined in the program prepared by the Nutrition Laboratory and submitted for criticism to over 400 physiologists, clinicians and psychologists a year or two ago. It is my purpose this evening to bring to your attention this program, its scope, its intentions, and more specifically to present to you in brief abstract the results of our first work on the psychological phase of the alcohol problem.

As the outcome of two extended foreign tours, when the general subject of physiological and psychological research on alcohol was discussed with many scientists, it became evident that:

1. Alcohol investigations are, as a rule, undertaken with diffidence, owing to the fact that relatively few investigators can afford the time or funds necessary to make the observations sufficiently numerous and extended to meet the stringent requirement of critics who, while frequently unscientific, are invariably captious.

2. Objectivity in writing on the subject of alcohol is as rare as uncontaminated scientific evidence.

3. Interpretations of the results of alcohol investigations made outside of the source of experimental evidence have been usually so confused by preconceived ideas of the reader as to lead to the most divergent interpretations of one and the same collection of data. In general an interpretation has been the resultant of scientific record plus the personal, ethical opinions of the reader, with the last named factor usually playing the controlling rôle.

Perhaps these discouragements to vitally needed abstract research on alcohol are after all blessings in disguise; to meet the demands of these adverse conditions tests the mettle of both the experimenter and the writer.

The rapid advances in physiological research, especially in the study of the energy factors both by direct and by indirect calorimetry, the availability of physiological instruments of precision, such as the string galvanometer, sphygmomanometer, electrical resistance recording thermometer, and appliances for the study of muscular work on an accurate basis, and an adequate technique for certain psycho-physiological observations made it seem feasible for the Nutrition Laboratory to begin a study of this general question, with the idea of using its resources and staff in such a manner as to convince all but the most captious of critics of the reliability of the data if not, indeed, of the legitimacy of the interpretation of results. To this end, and especially to secure a working outline which will correlate the immediate and later researches of all laboratories, a somewhat detailed program was planned.

In this program for experimental researches on alcohol the effect of only moderate doses is considered, since the effect of excessive doses with final complete narcosis is obvious. The importance of such a study of the influence of moderate amounts of alcohol is brought out in the introduction of the program.

Furthermore, emphasis was laid upon those points which in previous alcohol investigations had been most severely attacked. Thus, particular attention was given to the size of the dose, the character of the subject experimented upon, the question of repeated versus single doses, the

method of administration, *i. e.*, by mouth and by rectal enema, the time relations between the ingestion of alcohol and of varying foods as well as during fatigue, and the importance of securing adequate controls or base line measurements. This last point was especially emphasized, for in an alcohol investigation controls play as important a rôle as do the alcohol experiments.

A considerable part of the program is given to the general field of physiology, especially those factors which previous experience has shown might react more markedly to alcohol dosage. Studies of the respiration, digestion and secretion, metabolism, and the heat regulation are provided for. Finally, it was believed that since in studies of the metabolism the subjects would normally be under more or less control as to diet and alcohol ingestion, such investigations would provide an excellent opportunity for making simultaneous observations of the psychological effects of alcohol. A section of the program was therefore devoted to a plan for this research. The psychological part of the program was prepared by Dr. Dodge.

It is thus seen that the investigation to be carried out by the Nutrition Laboratory is based upon a carefully prepared program which has been submitted for criticism to a large number of individuals. That it will undergo material modification in the course of time is naturally to be expected, but with this program all completed researches may be correlated and profitable lines suggested for immediate attack.

The material resources of the Nutrition Laboratory of the Carnegie Institution of Washington, located in Boston, made it possible to commence upon this program immediately. The equipment of the laboratory includes several forms of respiration chambers and respiration calorimeters, the latter being placed in a room specially designed for studying heat measurements. Of the calorimeters the one most used is the bed calorimeter which was employed in the prolonged fasting experiment recently reported.[2] It is with these calorimeters that we expect to study many of the problems suggested in the tentative program. A portable form of respiration apparatus has also been developed and extensively employed in studies of the basal metabolism and in other researches in which the gaseous exchange is of special interest. At times no less than four such pieces of apparatus have been used simultaneously in the large respiration calorimeter room.

Without detailing further the equipment of the laboratory which may be used in the general study of the alcohol problem, we may immediately turn our attention to a specific discussion of the psychological section of the alcohol program. As Dr. Dodge was able to concentrate his entire time for a year upon this part of the work at the Nutrition Laboratory, sufficient data have already been accumulated to justify the publication of a monograph[3] on this phase of the subject. This work was carried out at the Nutrition Laboratory chiefly by Dr. Dodge and I should at this point state that when I say "we," I should more properly say "Dr. Dodge."

A special laboratory was prepared for the purpose with darkened walls to facilitate photographic technique and fitted up with psychological apparatus. The chief items of equipment consist of a string galvanometer and accessory apparatus, the electrically-driven Blix-Sandström kymograph, which is of general use, a complete equipment for the determination of the threshold for faradic stimulation (Martin

[2] Benedict, Carnegie Institution of Washington Publication No. 203, 1915.

[3] Dodge and Benedict, Carnegie Institution of Washington Publication No. 232, 1915.

method), and photographic apparatus with suitable luminants for registration of the protective wink reflex and of the eye movements. Both of the special cameras here used were designed by Dr. Dodge. Many other most ingenious devices for studying the problems arising in the course of the development of the psychological program were made by the experimental skill of Dr. Dodge and will be referred to later.

DESCRIPTION OF TESTS

Three fundamental principles determined our selection of the group of experimental measurements:

First, the attempt to secure a group of systematically coordinated measurements.

Second, the principle of relative simplicity, that is, we attempted to investigate elementary neuro-muscular processes in their simplest available form, and of the more complex processes to choose those involving as few unknown factors as possible. In particular we tried to measure processes that were insusceptible to direct and arbitrary conscious modification and as free as possible from uncontrollable influences of bias, effort and attention.

Third, and finally, we attempted to select those processes in which the motor response of the subject would be a thoroughly natural and familiar act, that is, a customary reaction.

The effect of alcohol on a relatively simple neural arc, the patellar reflex, has especial significance to clinicians who have long used it for diagnostic purposes. To render its study most capable of accurate scientific interpretation special apparatus was employed for giving the stimulus, and the latency of the response and degree of contraction of the quadriceps muscle was graphically recorded. The stimulus was given by two pendulum hammers of the same weight so placed as to strike the tendon. By means of a system of light levers fastening directly over the quadriceps muscle, the muscle thickening could be directly recorded on the kymograph and the shock of the impulse blow of the hammer made it also possible to record the moment of stimulus. From the moment of stimulus to the beginning of the rise of the curve, the height could be directly read and the extent of the quadriceps thickening could be obtained by noting the height of the curve and applying a simple factor.

Another simple neural arc which lends itself admirably for study is the protective lid reflex. By projecting a beam of light across the eyelid so that the shadow of an eyelash will fall upon a sensitive plate moving horizontally in a camera, a complete photographic registration showing the moment of stimulus, together with a picture of the lid movement, was obtained. The stimulus was produced by a sounding board struck by two spring hammers, the whole stimulus system having a small pointer whose shadow likewise fell over the camera slit, and thus was simultaneously photographed. As the moving plate was set in motion the stimulus hammers were electrically released and the moment of stimulus and the beginning of lid movement, as well as the height of response, were photographically recorded. It is extraordinarily difficult for a subject to alter the nature of the protective lid reflex or of the knee jerk without this alteration being evident in the records, and we have here, we believe, two indices that give us as nearly uncontaminated data as one could at the present time expect.

Of the complex neural arcs the movement of the eye to a peripheral visual stimulus is one of the common experiences of everyday life. Consequently, by means of the Dodge photographic registration of the movements of the eye, it was possible to record photographically the time required

for the eye to react, *i. e.*, to look over to a new stimulus to the right or to the left of the position which the eye has been occupying. The subject was placed with his eye near the lens of a long photographic camera; a beam of soft blue light from an arc lamp was then projected at will upon the eyeball and reflected through the lens, which focused it to a small point upon a sensitive plate in the camera. The eye looked at a fixation mark; this suddenly disappeared, exposing in an unknown position a single letter which was 1, 2 or 3 centimeters to the right or to the left of the fixation mark. At the moment of exposure the actinic light was allowed to fall upon the eyeball. A tuning fork interrupted the light so that the record consists of a line of dots, each dot representing one hundredth of a second. A bend in the line indicates the moment of reaction, thus giving a photographic record of the latent time as well as the direction of movement.

A second complex neural arc was the reaction time to reading isolated words. With an exposure apparatus of unique advantages 4-letter words were exposed, and the subject was instructed to pronounce each word immediately as soon as he saw it. By means of a voice key with electric contact a record could be made on the kymograph drum of the moment of exposure of the word and the instant of response, thus giving the data for determining the latency of response.

In conjunction with Dr. F. L. Wells, of the McLean Hospital, a series of standard free association tests were given to the subjects. The procedure is so well known as to make a description unnecessary here.

Considerable attention was paid to the effect of alcohol on memorizing, this being one of the higher mental processes. A series of 4-letter words were exposed backwards by attaching to the kymograph drum a piece of paper on which these words were printed. As the drum rotated the last letter of the word appeared first in the window which limited the subject's view, and not until the last letter had in turn appeared could the subject pronounce the word correctly. The words were repeated three times and the saving in time required to repeat the words in order was taken as an index of residual memory. Occasionally subjects could memorize the entire twelve words so as to have a perfect score, pronouncing each word before it was exposed.

The effect of alcohol on the sensory threshold was studied by means of the faradic stimulation method of Martin. This requires a carefully calibrated Kronecker inductorium; the technique has been admirably described by Dr. Martin.[4] Very feeble shocks were produced and the subject, with two fingers dipped in salt solution, was told to indicate when the shock was felt. By moving the coil in and out a threshold could be established. Variations in the position of the coil were the basis of a series of calculations which Dr. Martin has developed, giving the results in the terms of certain units. Since our work is entirely differential, however, it is unnecessary to discuss the character of these units.

No series of observations on the psychological effects of alcohol would be complete without studies of the effect of alcohol on motor coordination. After a considerable amount of preliminary experimentation we decided upon two measurements, the first on the velocity of eye-movements, *i. e.*, in moving the eye from side to side through an arc of about 40°, the second on the reciprocal innervation of the middle finger. The importance of the eye as a member for studying muscular coordination has been

[4] Martin, "The Measurement of Induction Shocks," New York, N. Y., 1912.

emphasized by Sherrington and has been given special attention by Dodge. It is relatively impossible to control voluntarily the rate of moving the eye from one point to another, as this invariably results in an intermittent motion. By having two fixation marks and, on signal, moving the eye as rapidly as possible from one to the other, a photographic record was made of the eye movements. The time is recorded directly on the plate as in eye reaction records, by means of the light interrupter. From this record we count up directly the number of hundredths of a second required for the eye to move through this arc of 40°. This time or velocity measurement is the basis of our discussion. Of course the record indicates the accuracy with which the eye looks at these two marks and also the number of times that the eye moves from mark to mark. This technique has proved most satisfactory.

Since eye movements are not well adapted to show the rapidity of free oscillation, as successive eye movements are separated by moments of fixation, we adopted the reciprocal innervation of the middle finger for measuring the speed of alternating reciprocal innervation of antagonistic muscles. The finger movements were recorded in the following manner:

A very light lever, the weight of which was not borne by the finger, but held by a pivot, was attached to the middle finger of the right hand and the projection of the lever placed opposite the slit of a photographic camera, so that the shadow of the lever could be photographed directly. At the same time electrocardiograms were taken from body leads, thus giving the pulse rate; a respiratory curve and the time in seconds were simultaneously photographed. The number of finger movements during the first, second and third 2-second periods were the basis for discussion. Even in this short time distinct evidence of fatigue was noted.

With these important measurements of simple and complex processes, memory, electrical threshold, eye movements, and finger movements, a large number of pulse measurements were taken throughout the series of observations. After making tests with various forms of apparatus, we finally relied entirely upon electrocardiograms taken from body leads, two electrodes being attached to the chest, thus leaving the subject entirely free and untrammeled for any other simultaneous tests. The electrocardiograms were obviously not taken for diagnostic purposes and have value here as showing only the pulse rate. As the measurements were made not only during the mental work, but during moderate amounts of physical exercise, such as finger movements, rising, and two genuflections, the data obtained permitted us to draw conclusions regarding the heart rate during moderate mental and physical work.

In considering all of our experiments it is important to note that the processes selected for measurement are, we believe, for the most part remote from voluntary or conscious modification or control. The program was quite varied, the subjects passing from one series of tests to another, yet the apparatus was so disposed in the laboratory as to necessitate a minimum amount of extraneous muscular movement in changing from one position to another. Special care was taken to have the whole atmosphere of the laboratory quiet and serene.

SUBJECTS

The problem of collecting subjects for a research of this kind was by no means simple. It was finally decided that a good degree of intelligence was necessary for proper cooperation between subject and experimenter; furthermore, the moral respon-

sibility of giving irresponsible individuals even the distasteful mixtures of alcohol and water was not lightly to be regarded. We therefore decided that only those individuals who were of legal age, and who were college graduates, would be used. Consequently we had approximately ten such subjects and, through the kindness of Dr. E. E. Southard, these were supplemented by a group of three men from the Psychopathic Hospital in Boston to give some indication of the influence of previous excessive use. Our final deductions were drawn from an elaborate series of figures based upon approximately 7 normal and 3 psychopathic subjects, but by far the more extensive series of tests was carried out with the normal individuals.

METHOD OF COMPUTATION

At the outset it was emphasized that an extended series of normal or control experiments was absolutely essential. In fact, we made approximately as many control experiments as we did alcohol tests. This is believed to be good practise as the question of interest, fatigue and the invariably occurring but unforeseen rhythmic and arrhythmic changes throughout any given experimental day must be counterbalanced by control experiments of an extensive nature. Consequently the general method of experimentation was somewhat as follows:

First, the subject was given an entire normal day (about 3 hours) without alcohol. Later this session was duplicated, with a 30 c.c. dose of alcohol. At a third session the dose was raised to 45 c.c. and finally the series was concluded by another control day. In addition to the control tests at the beginning and end of the series, the first period of each alcohol day was given without alcohol, so that we have a "normal of the day," that is, the entire group of measurements for that given day was carried out once before alcohol was given and then subsequently alcohol was administered. The series of observations was repeated several times, depending upon the length of session; usually there were three and sometimes as many as six repetitions on a single day. To allow for the normal daily change, which was invariably noted in all repetition of sessions, we deducted from the "normal of the day" the values found in subsequent series of that particular day; these differences were the basis of our computation. A comparison was then made of differences for the two alcohol days with those for the two normal days; the effect of alcohol was represented by the difference found between the alcohol and the normal days. The statistical treatment of the subject gave us no little trouble; obviously the data permit of any other adjustment or rearrangement that statisticians may deem advisable.

RESULTS OF COMPUTATIONS

Considering only the effect of alcohol with the normal subjects, that is, the difference between the differences found on the alcohol days and those found on the normal days, we note that the latent time of the patellar reflex was lengthened on the average about 10 per cent. In other words, after the stimulus the response in the contraction was delayed approximately 10 per cent. The extent of muscle thickening was enormously reduced, this reduction being, on the average, 46 per cent. In fact this diminution in the muscle thickening was so great as to make it impracticable to conduct the experiment with most subjects after giving the 45 c.c. dose.

The second reflex studied, namely, the protective lid reflex, showed that the latent time was increased 7 per cent., while the extent of lid movement was decreased 19 per cent., a picture perfectly comparable with that of the patellar reflex.

The latent time of the eye reaction, that is, the time elapsing between the disappearance of the fixation point to the movement of the eye appropriate to the stimulus, was increased 5 per cent. In other words, the reaction was distinctly delayed.

In the speech reaction tests, in which the subject was asked to pronounce a word shown in the exposure apparatus, the latent time was increased 3 per cent.

Memory and the free association were only slightly affected. The sensitivity to faradic stimulation was decreased 14 per cent. after alcohol, that is, the threshold was raised, a stronger shock being required to stimulate. In the motor coordinations the number of finger movements in 6 seconds decreased 9 per cent. with alcohol, and the velocity of the eye movement through an arc of 40° decreased 11 per cent., *i. e.*, there was a decrease in velocity both of finger movement and of eye-movement. Thus we see that all the tests thus far show a distinct depressive action of alcohol, a minimum effect being observed in the more highly organized processes, such as memory and the free association.

The large number of pulse observations made during mental work and during moderate physical work showed invariably a relative acceleration of the pulse; this is in striking contrast to the general depression of the neuro-muscular processes at all levels of the cerebro-spinal system. To make this clear, it should be stated that during the sessions of a normal day the pulse rate would tend to decrease with fair regularity from hour to hour. For example, the average pulse rate for the first half hour might be 80 per minute and the rate might decrease until the average for the last half hour would be 62 per minute. On the normal days this picture is very clear. On the alcohol days this decrease is not nearly so large as on normal days. In no instance is the pulse rate higher at the end of the day than at the beginning, but the normal drop does not occur. In other words, we have here distinct evidence of a relative acceleration. Under all conditions of moderate mental and physical work the pulse rate after alcohol did not fall to so low a level as it would have fallen without alcohol.

Summarizing the effect of alcohol we find, then, that the two reflexes showed a distinct lengthening in the latent time of 10 per cent. for the patellar reflex and 7 per cent. for the lid reflex, a decrease in the muscle thickening for the patellar reflex of 46 per cent. and in the extent of the lid movement of 19 per cent.; the sensory threshold was raised 14 per cent.; the two motor coordinations showed a decreased action under alcohol of approximately 10 per cent.; the two elaborated reaction times of the eye and speech organs showed changes of 5 and 3 per cent. respectively, and the memory and free association were unaffected. The natural grouping of these processes is too consistent to be accidental. It is confirmatory evidence of the reliability of our results that similar processes gave similar results. Furthermore in the cases in which there are comparable data it is shown that in 5 out of 6 processes the greater average effect was the result of the larger dose.

It is surprising that the higher processes were apparently not so much affected as the reflexes, but it is significant that the greatest, most persistent change incidental to the ingestion of alcohol is in those processes which are most distinctly exempt from voluntary reinforcement or voluntary control. The higher senses alone show capacity for autogenic reinforcement. This we noted several times in the progress of our experimenting. One most striking illustration was when one of the subjects, during the association test, went to sleep for a

few seconds, and failed to hear one stimulus word; ten seconds later, after being awakened, he responded normally both as to latent time and the character of the associated word. There was a distinct effort in all cases for the subject to attempt to pull himself together to make a showing, but it is a striking fact that in spite of the autogenic reinforcement, with one exception the performance after alcohol was below normal. The one exception was the eye reaction to visual, peripheral stimuli after the 30 c.c. dose, where there apparently seemed to be facilitation as the result of the moderate dose of alcohol. An examination of the data for all the subjects shows, however, that there was a marked practise effect, which was entirely unlooked for in the arrangement of our experiments. But even this could not offset the effect of the larger dose, which invariably showed depression of the reaction.

In the analysis of our data it becomes necessary to consider all sensory and motor processes as the resultant of complex, stimulating and inhibiting factors. It was distinctly noted in our experiments that when there were definite inhibitory processes, such as, for example, in one or two instances where the protective wink reflex was inhibited as the result of training, that this inhibition suffered first under alcohol. This was also noticeable in the threshold for faradic stimulation where alcohol disturbed the subject's caution and produced more numerous false reactions, that is, reactions when there were no stimuli. We have, on the one hand, with the higher senses a capacity for autogenic reinforcement, and, on the other hand, a tendency for the alcohol to affect inhibition. It seems not unlikely that we have here a partial explanation at least for the wide variety of effects which are commonly observed in the social use of alcohol, where environment gives the reinforcement and alcohol reduces the inhibitions. Our evidence is positive, however, that the ingestion of alcohol results in a depression of neuro-muscular processes and that these phenomena can not be reduced to the excitation of the inhibitory processes. But conversely, whenever an apparent excitation occurs as a result of alcohol it is either demonstrably (as in the case of the pulse rate, the reflexes, memory and the threshold) or probably (as in the eye reaction) due to a relatively overbalancing depression of the controlling and inhibitory processes.

The most apparent exception to the general trend of depressions noted in all the processes was that of the pulse. A careful analysis of individual pulse cycles showed, however, by the method of Reid Hunt, that there are large variations in the length of diastole after alcohol. Since many of our experimental observations were made in relatively short periods, it appeared logical to assume that the changes in pulse rate were due not to the stimulation of the slow-acting accelerating mechanism, but to the rapidly reacting inhibitory mechanism. We may therefore explain this apparent relative acceleration of the pulse rate on the ground of a partial paralysis of the cardio-inhibitory mechanism.

It is also of interest to note the time of the maximum effect of alcohol in the various processes. In general there was a remarkable uniformity in the time after the ingestion of alcohol when the greatest effect was noted. This was practically from 90 to 100 minutes, in other words a little over an hour and a half, and essentially constant for all the processes.

Finally, to note if there was a central tendency in any one of the particular series of measurements we compared the average effect of alcohol upon each process for the individual subjects with the average for the

group and found that the average effect was closely approximated by the effect upon the motor coordinations, *i. e.*, finger movement and eye movement. Aside from the practical value of this correspondence between the effects of alcohol on the coordination processes and the average effects, it has a rather far-reaching, theoretical implication. If, in all the diverse processes that we have measured, the coordination processes represent a central numerical tendency, it must be that they correspond in some closer way than the rest to the real central tendency of the alcohol effect. It would seem to indicate that the change in the average performance of our subjects due to alcohol was a function of central coordination. If this indication is substantiated by later investigations it should prove to be not only of the utmost importance for the understanding of the various manifestations of the effects of alcohol in individual cases and of the general phenomena that accompany its excessive use, but it would throw a flood of light on the complex organization of normal psychophysical processes as well as on the effects of fatigue and other depressing agents.

In reporting these results I feel that the techniques outlined above have been adequate to this type of problem, that the controls have been satisfactory, and that the data and interpretations have been presented with the least amount of personal bias that is possible to scientific investigators. But naturally these results must not be considered as having settled the alcohol problem as regards the psychological effects of small doses. As all present know, the literature in this field is very large and there are numerous contradictions in results and interpretations. It is very rarely possible for one investigator to duplicate the apparatus, methods and general conditions of another. This is doubtless one of the main causes for confliction. We therefore feel that one of the important contributions to our program and scheme of work on the alcohol program should be the continued massing of data over a period of years. It is also advisable that the data should not be collected by one experimenter. If two experimenters, both carefully trained, and thoroughly familiar with the same group of methods should use these methods on a different set of subjects, but under similar laboratory conditions, comparisons of these two sets of results should be of great importance. If they are opposed to each other, it is a good index of unknown or uncontrolled factors in the problem, insufficient data, carelessness of technique, or wrong statistical treatment. On the other hand, should these results confirm each other in the main, and especially if both groups of data are fairly large, this confirmation should place these results upon a plane which is unique in the annals of alcohol experimentation.

It will doubtless be considered of enormous practical significance that in none of our data have we any indication of the pure facilitation effect of alcohol. Contrary to the theory of Kraepelin, we not only found no facilitation of the motor processes, but the depression of the simplest forms in the finger and eye-movements seems to be one of the most characteristic effects of alcohol. Certain it is that, in conjunction with the pulse acceleration, the general neuro-muscular depression may be regarded as presumptive evidence of the effect of alcohol on organic efficiency. It is, however, of vital importance in seeking to transfer the results of such laboratory demonstrations as have been here reported to a general consideration of alcohol on industrial efficiency to recall that the higher central processes by reason, we believe, of autogenic reinforcement showed the least effect. Indus-

trial processes are by no means solely confined to motor coordination, and I must emphasize that the data of this report may not be uncritically applied to industrial situations. More complex processes, such as typewriting, which seem to apply more directly to industrial environment, are being studied, and their various factors analyzed by my colleague, Dr. Walter R. Miles, experimental psychologist of the Nutrition Laboratory. It is only upon the basis of such analysis that justifiable conclusions may be made with regard to the effect of alcohol upon the mental and physical demands of industrial environment.

It may be added that the material already published is being further elaborated, both experimentally and statistically, by Dr. Miles.

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CINCHONA AS A TROPICAL STATION FOR AMERICAN BOTANISTS

IT is now practically assured that some fourteen American universities, botanical foundations and individual botanists are to cooperate with the Jamaican government in the support of Cinchona as a tropical station. A move to aid in the support of Cinchona, initiated by the Botanical Society of America in 1912, was not consummated, in consequence of the earlier leasing of the station to the British Association for the Advancement of Science. The Jamaican authorities and the British Association seem quite willing, under present conditions, to allow the lease to pass into American hands after October next.

The attention of American investigators should, therefore, be directed to the facilities for botanical research offered by this oldest and best known botanical laboratory in the western tropics.

A brief description of the location of Cinchona and of its botanical environment has recently appeared in *The Popular Science Monthly* (December, 1914, January, 1915). Among the advantages of this station for American botanists there enumerated are the greatly varied flora and series of types of vegetation; the proximity of a library and of two other botanical gardens, beside that surrounding the laboratory. The location of Cinchona is a very fortunate one for American botanists from a practical standpoint. It is in an English-speaking country with good roads, a stable government and adequate quarantine service. It is also within easy reach of our eastern seaports, from several of which the round trip to Jamaica and Cinchona can be made in summer for $75.00 or less for transportation.

In order to give a more adequate idea of the advantages of Cinchona for several different types of research I have asked four investigators who have worked there to suggest the opportunities presented at Cinchona for research in the four or five lines which they have followed. These outlines are appended under the names of their respective authors.

It is altogether probable that any American botanist wishing to work at Cinchona during the coming summer will be granted the privilege by requesting it of the colonial government of Jamaica through Superintendent William Harris, F.L.S., Hope Gardens, Kingston, Jamaica. The writer and the authors of the appended notes on the botanical opportunities of Cinchona will be glad to give any information, within their knowledge, of conditions at and about the laboratory.

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May 25, 1916

THE FERN-FLORA OF CINCHONA

THE writer has visited a good many regions rich in ferns, but none equals Jamaica either in number of species or individuals. The extraordinarily varied conditions in Jamaica, due largely to its topography, result in a variety of the fern flora which is really amazing. About five hundred species are reported from this small island, of some four thousand square